Hydrol. Earth Syst. Sci. Discuss., 8, C684–C689, 2011
www.hydrol-earth-syst-sci-discuss.net/8/C684/2011/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Hydropedological insights when considering catchment classification" *by* J. Bouma et al.

K. Beven (Referee)

k.beven@lancaster.ac.uk

Received and published: 30 March 2011

Review of Bouma et al. Hydropedological insights when considering catchment classification

I have a huge respect for what Johan Bouma, in particular, has done in advancing the case of soil hydrology and hydropedology in real practice (rather than the theoretical niceties but not very realistic practice of soil physics) but I really do wonder what insights this paper is providing since there is so much that is left out. The authors are suggesting that hydrological modellers and those interested in catchment characterization are not taking soil information sufficiently into account but they make no useful suggestions about how they should do so, including in the Case Studies they present (all of which have been published elsewhere). The first uses SWAT in a large scale

catchment in Kenya. This requires either estimating curve numbers for different HRUs as if they are homogeneous, or using Green-Ampt parameters (but without real information on Ksat) as if the HRUs are homogeneous. The model is then calibrated (ironically, given some statement elsewhere, such as Page 2157 Line 1 ff, against discharge data!!). In what way is that either good hydrological or hydropedological practice? It does allow predictions to be made to help solve a very practical problem (although even then why should curve numbers and the adjustments made for contour stripping developed in the mid-West of the US apply without change in Kenya?).

In Case Study 2 some allowance is made for bypassing flow on the basis of 18 dye experiments. That is not enough samples to even get an estimate of mean soil moisture with limited variance in a field, so why should we have any belief that it is adequate to scale up to hillslope and catchment scales. The same applies to the preferential flows in non-wetting soils (and soils do not have to be non-wetting to see this type of preferential flow behaviour as is now demonstrated by many lab and field tracing experiments). The problem with soils information (both in old-fashioned mapping and in the specification of dynamics) is that we cannot see enough of it – and if we dig to get a better view we not only see a very very small sample (a huge disadvantage) but we also change the characteristics at that point (in which case it is an advantage to only have a small sample). Ideally, of course, we would take our samples at truly representative sites – except that we do not know where they are, and the whole process might be undermined by some larger scale process (undulating bedrock, pipes, iron-pans that vary dramatically in depth, large periglacial structures not readily revealed by an augur or even a pit . . .. . .). Look at the very simple problem of trying to study the influence of soil depth (within one relatively homogeneous soil pedon / HRU) at the small Panola catchment and its effect on the hydrology (see Freer et al. WRR2002).

So we have to generalize in implying larger scale behaviour (soil mapping units so often following topographic contours) or pedotransfer functions. . .. . .actually originally driven largely by hydrologists rather than soil scientists. The authors suggest that pedotransfer functions might still be useful but I am not exactly sure why. The data on which they are based is mostly from measurements on very small samples (Johan Bouma, to his great credit, instigated a program of larger (but still small) block measurements in the Netherlands and there have been occasional efforts using larger undisturbed cores elsewhere, but this has not been much repeated because of the sheer effort involved). So what hydrological parameters do pedotransfer functions provide? Estimates (actually highly uncertain, although the uncertainty is often ignored) of point scale values of Ksat, AWC, soil moisture characteristic parameters etc. And is the uncertainty associated with those estimates indicative of the heterogeneity that might be found in a soil unit, texture class, etc. No, because it is the standard error of some “best” estimate which is something quite different.

And if there is heterogeneity that leads to preferential flows in the unsaturated zone what does that mean? It means that we should not be using Richards’ equation to represent the larger scale because that is nonlinear and will not therefore average to work with some effective large scale parameter values. We should be using some other representation. This has been known for 30 years or more but mostly ignored because nobody could suggest a simple representation that had a good theoretical (rather than conceptual/symbolic) basis. But as the authors suggest, there are now animations that show how these local scale interactions work. Great, but how can I use these at the field, hillslope or catchment scale without any idea what the underlying heterogeneity might be. Can they be used to suggest new representations – certainly, but with different definitions of parameters for which we do not have readily available information. Might such a new representation look like the SWAT profile redistribution with flux as a function of water content + perhaps some bypassing - perhaps but certainly not with a point scale estimate of Ksat.

In Case Study 3 they suggest that using spatial estimates of actual evapotranspiration derived from SEBAL can be an enormous aid in the calibration of SWAT. I would agree but would make two points. The analysis seems to take no account at all of the

uncertainty of the SEBAL estimates of actual evapotranspiration which can be large and comes from multiple sources in closing the energy balance (even from the original thermal infrared images – the digital numbers have already been processed through a deterministic interpretative model with parameters that are uncertain and time varying). The second is that this information would be useful in calibrating a distributed model anyway even if no real attempt was made to include hydropedological information – and in fact their explanation of most of the anomalies in the estimates are examples where this was not done adequately. Perhaps there would be more anomalies if less care had been taken about setting up the distributed fields of parameters, but there is a really important general lesson here.

That is that wherever we have the information - but ONLY when we have the information - we can learn from finding out that something is wrong. That is what Case Study 3 demonstrates (this approach has been formulated previously in Beven, HESS2007). Thus all catchment classification, GIS, hydropedology, and other information is really only a way of getting prior information of what catchment characteristics might be. I actually know of NO hydrological modeller who is NOT interested in having good soil parameter information in setting up a model*** – the problem is whether it is really providing the effective values of parameters (that might be different for different element scales, model implementations etc etc) to give good simulations – ideally WITHOUT calibration (NOT the case here, and very very few papers anywhere have suggested success in doing so despite often taking soils information into account). However, as soon as the model predictions (whether of discharge, patterns of evapotranspiration, patterns of water table, saturated contributing areas etc) are proven to be inadequate we SHOULD be moved to improve the model. All the time it seems to be working we will be happy...... but with a nagging doubt that there is still a possibility of getting the right result for the wrong reason. So we can end on a really positive note – the hydropedologist, on the basis of the information she can provide about genotype and phenotype morphology, might help ensure that models might be getting the right results for the right reasons.

Hydropedology is therefore undoubtedly important but so many issues have been ignored in making the case here. The question that the authors need to address therefore is how that might be done (perhaps using the simple example of the effect of variations in nothing more complicated than sub-HRU variations in soil depth on flow pathways and transpiration rates). They have stated the issue correctly *"However, as stated above, the soil forming factors and associated processes that have resulted in the morphology of a given soil profile do not necessarily represent actual physical, chemical and biological processes that determine soil behaviour. A functional analysis is therefore needed to "translate" properties of a given soil type into actual dynamic processes determining soil behaviour"*

The solution is certainly not, however, in the ways suggested here. Can I suggest that the authors be much more forward looking in suggesting ways of overcoming the current limitations of soil characterizations in informing about catchment dynamics (and I really do not accept that SWAT is the key!!) – otherwise some hydrologists may continue to concentrate on analysis of discharge data (which is, after all, a rather good integral summary of catchment dynamics). Consider the question – what would be more cost effective in determining the response of an ungauged catchment – a survey of soil morphology, some measurements of hydraulic conductivity, or a few hydrographs by putting in a cheap level sensor and making an equivalent number of discharge measurements? Ooops! That is not quite so positive is it?

However, there is still the case for major revision rather than rejection. We all know that soils information is important in catchment dynamics (cases such the bocage in Britanny are all too obvious, but very often it is much more subtle) - so we need a guide as to how to really make use of it - to have the right type of translation from the GIS overlay (and/or hydrologically relevant survey) to dynamic processes. This paper does not yet throw much light on how to do that but I would very much hope indeed that a future version might do so.

Keith Beven

*** Some remarks in the paper might be considered insulting. There are very many hydrological modellers (surely the majority, even if not all follow good practice, for example in predicting the impacts of change) who are concerned with illuminating societal problems (including the original developers of SWAT and its predecessors concerned with improving agricultural management) and the limitations of matching observed discharges and doing better hydrological science have been extensively discussed in papers not cited here. It is also the case that hydrological modellers really do understand that antecedent conditions, patterns of rainfall, dynamics of contributing areas etc do play a role in determining the discharge dynamics and it is really rather hard to see how the authors should have another impression.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 2145, 2011.